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Abstract

This paper argues that fictionalism about folk psychology, FaF, is ill motivated in any domain. It is argued that there is no advantage in trying to vindicate folk psychology by treating the constructs of classical cognitivism—namely, subpersonal mental representations—as useful fictions in contrast to serious scientific posits or as serving as the basis for philosophical explanations. Both scientific and philosophical considerations point to the conclusion that subpersonal representations of the sort that classical cognitivism posits should be eliminated, not preserved, by our best science of mind. Yet there is no need to assume that folk psychological explanations are subpersonally based. It is possible and plausible that such explanations are based, just as they appear to be, in nonscientific interpretative, narrative practices. A recent attempt to motivate FaF based on this assumption is examined and rejected. Then a more compelling, Dennett-style rationale for adopting a FaFish line based on worries about the indeterminacy of folk psychological attributions is considered. Dennett endorses FaF, broadly construed, in arguing that while folk psychological phenomena exhibit objective patterns, they are nevertheless, at best, mildly real. The final section of the article offers three considerations that should encourage the reader to resist such FaFish conclusions.

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FICTIONALISM ABOUT FOLK PSYCHOLOGY

For nothing is actually removed from existence by being labeled . . . appearance . . . What appears is there, and must be dealt with; but materialism has no rational way of dealing with appearance.

—Bradley
Appearance and Reality, 1930, 12.

1. Introduction

Fictionalists avoid commitment—full-fledged ontological commitment, that is. To be a fictionalist about a given domain is to adopt a strategy of taking discourse about the domain seriously while at the same time stopping short of accepting that such discourse entails any, potentially embarrassing, ontological commitments. Endorsing fictionalism about some domain is thus meant to provide a way of taking a less-than-fully realist stance towards the domain's entities without rejecting the value or usefulness of discourse about the domain altogether. Sainsbury highlights this as the main motivation of fictionalism:

the starting point for fictionalism is some kind of ontological scruple: one cannot bring oneself to believe in moral values, nonactual things, unobservable things, or abstract things. But one has somehow to do justice to the fact that one cannot simply throw away the related regions of discourse: morality, modality, elementary physics, or mathematics. Fictionalism to the rescue. (2010, 2)

This paper argues that fictionalism about folk psychology, FaF, is ill motivated in any domain. It is not a successful way of dealing with eliminativist threats aimed at classical cognitivist theories—theories which predict that folk-psychological constructs, understood subpersonally, will be vindicated by a mature cognitive science. Nor would FaF be an appropriate response to worries about the ontological status of mental states and the determinacy of their content, where such mental states are understood

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as featuring in reason explanations about the actions of people. The paper's moral? Friends of folk psychology are best advised to expose such threats as empty rather than by making fictionalist ontological concessions.

The case against FaF is made in the following steps. It is argued that there is no advantage in trying to vindicate folk psychology by treating the constructs of classical cognitivism—viz. subpersonal mental representations—as useful fictions as opposed to serious scientific posits or as serving as the basis for philosophical explanations. Both scientific and philosophical considerations point to the conclusion that subpersonal representations of the sort that classical cognitivism posits should be eliminated, not preserved by our best science of mind (Section 1). Yet there is no need to assume that folk-psychological explanations are subpersonally based. It is possible and plausible that such explanations are based, just as they appear to be, in nonscientific interpretative, narrative practices. A recent attempt to motivate FaF based on this assumption is examined and rejected (Section 2). Then a more compelling, Dennett-style rationale for adopting a FaFish line based on worries about the indeterminacy of folk-psychological attributions is considered. Dennett endorses FaF, broadly construed, in arguing that while folk-psychological phenomena exhibit objective patterns they are nevertheless, at best, mildly real (Section 3). The final section offers three considerations that should encourage the reader to resist such FaFish conclusions (Section 4).

1. Fictionalism about Folk Psychology: Take One

Classical cognitivism characterizes subpersonal cognitive states in a way that is deeply indebted to our everyday, folk-psychological conception of mind. Ramsey (2007) exposes classical cognitivism's commitment on this front, showing how it gives "rise to an outlook on representation that amounts to a sort of merger between classical computational theory and folk psychology" (Ramsey 2007, 38). Why so? What's the connection? On the one hand, cognitivism embraces the view that "cognition is computation, which [is] . . . understood as a form of quasi-linguistic-symbol manipulation done in accordance with specifiable rules" (39). On the other hand, cognitivism also regards what is processed as essentially representational: the mental contents that computational processes manipulate represent the world as being in certain ways. Thus the mind both represents and computes. Given this, Ramsey astutely asks:

What type of representational notion is invoked in computational explanations of cognitive processes? The answer proposed is, the same type of representational notions that are invoked by commonsense psychology. Computational processes are seen as a mechanized version of folk-psychological reasoning, and this is only possible if the symbols being manipulated are viewed as analogues of familiar commonsense mental representations. (2007, 45; see also 46)

It is therefore no accident that the mental states and processes that cognitivism takes to define the very essence of the psychological are “scientific analogues for beliefs, desires, ideas, thoughts, and similar representational posits of folk psychology” (2007, 38; see also Machery 2009, 42). Speaking on behalf of classical cognitivism, Seager tells us that “The whole thrust of cognitive science is that there are sub-personal contents and sub-personal operations that are truly cognitive in the sense that these operations can be properly explained only in terms of these contents” (1999, 27). Anyone committed to such a view will assume that the fate of folk psychology directly hangs on the outcome of debates in cognitive science.¹

With this in mind, eliminativists about folk psychology anticipate a brave new world. They predict that—one day—cognitive science will rid itself of all vestiges of everyday ways of making sense of ourselves in terms of beliefs, desires, and other mentalistic attitudes. For them, doing so will be a major step forward because, by their lights, folk psychology is a false theory of mind that only warrants wholesale rejection or elimination.² They wager that some day it will be shown that the explanatory posits of a mature scientific psychology will not resemble those of folk psychology in any relevant respects. Placing the opposite bet, classical cognitivists believe that folk psychology’s core commitments (or something close enough) will be shown to fit with our best scientific psychology. Both sides assume that “Folk psychology is committed to the existence of mental representations. Therefore, for folk psychology to be vindicated, the correct scientific theory needs to invoke, at the very least, . . . cognitive representations” (Ramsey 2007, 114).

Who’ll win? Revolutionary developments in cognitive science make eliminativism the bookies’ favourite. Why? Compelling reasons have emerged for thinking that cognitive science will, in the end, adopt the view that subpersonal cognition is not fundamentally contentful or representational—at least not in the way required for it to vindicate folk psychology. As

Ramsey observes, “something very interesting is taking place in cognitive science . . . cognitive science has taken a dramatic anti-representational turn” (2007, xiv–xv).

The complete adoption of nonrepresentationalism would be a major sea change, a fundamental shift in thinking in cognitive science. But such change appears to be on the horizon. Antirepresentationalism is rapidly gaining ground. It is motivated by (i) growing scepticism about the utility of representational talk in predicting and explaining the behaviour of cognitive systems in conjunction with (ii) serious worries that no tenable theory of informational content will be forthcoming that could justify such representational talk in any case (see Hutto and Myin 2013; Chemero 2009, 77).

For the sake of argument let us suppose that this scepticism and these worries are justified. Let us assume that, in the end, it is agreed that there are no such things as subpersonal mental representations with contents. Might FaF be an option for classical cognitivists who reluctantly accept this but still want to resist thoroughgoing eliminativist conclusions?

Put another way: Is it possible to deny that contentful subpersonal mental representations really exist while allowing, nevertheless, that quantifying over such things is ineliminably important for explanations of mature cognitive science? Clearly, to take this line it won’t do to simply hold that talk of mental representations is useful shorthand—as many scientists often propose. Accepting this might provide grounds for thinking that it would be practically inconvenient to get by without talk of mental representations. But that is not a strong enough reason for retaining such talk in a mature cognitive science; it would provide no basis for thinking that such talk is truly ineliminable in the appropriate sense.

A stronger FaFish line of reply might be warranted if it could be convincingly established that representational talk plays a critical, instrumental role in enabling predictions and explanations in cognitive science. This would be a respectable way of defending the view that talk of mental representations is necessary in cognitive science, and thus theoretically indispensable. Taking this line would be consistent with holding that such phenomena should be taken no more seriously ontologically than we take centres of gravity, centres of population or other calculation-bound entities. Mental representations might be thought of as *abstracta* as opposed to *illata*. Accordingly, mental representations might be thought of as having

a metaphysical status akin to nonexistent objects that we must mention and quantify over when making certain kinds of predictions or calculations.

But instrumentalism about mental representations looks, at best, like wishful thinking. This will be so as long as antirealism about mental representations is motivated by worries that mental representations are wholly redundant in the predictions and explanations of cognitive science. And this is precisely the worry that has been voiced. Thus:

despite the fact that one can cook up a representational story once one has the dynamical explanation, the representational gloss does not predict anything about the system's behaviour that could not be predicted by dynamical explanation alone. (Chemero 2009, 77; Shapiro 2011, 147)

Taking this a step further, Ramsey provides an insightful analysis of why scepticism about the explanatory value of talk of subpersonal 'contents' and 'representations' is justified. He observes that there is a widespread assumption that informational content is being carried by covarying dependencies that are relevant to the proper functioning of internal structures. This, he holds, is:

the main motivation behind the common tendency to *treat things that respond as things that represent*. If neural structures are actually recruited as causes of bug-catching movements because they are reliably caused to fire by the presence of bugs, then it certainly seems tempting to assume that they are serving as bug representations. The question we need to address is, *does this arrangement suffice for something to serve as a representation?* (2007, 133, emphases added)

On careful review of the relevant scientific literature Ramsey's answer is, except for certain special cases, unequivocally: 'No'. But, he argues, it is not just that representational interpretations are explanatorily superfluous, unwarranted, and unnecessary. Ramsey provides evidence that they have proven detrimental in some cases. This is hardly surprising for "if you assume that something plays a role that it actually doesn't play, then your functional analysis of the system is going to be off the mark" (2007, 147). He illustrates the point with the following example:

Research on the olfactory system was derailed for several years because they mistakenly assumed that certain neural arrays functioned to represent olfactory stimuli because of the way they responded to that stimuli. This interpretation led them away from a more accurate understanding of the roles the neural states play in brain dynamics. (2007, 147)

Still, there might be another way to motivate FaF. For it is possible to accept all of this while taking a McDowellian line about subpersonal contents. McDowell holds that talk of contents being exchanged between subpersonal systems is “irreducibly metaphorical” (1994/1998, 349). He openly accepts that there are no such things as subpersonal contents. Nevertheless, he speaks of the “enormous capacity for illumination that the ‘sub-personal’ account has” (349). With rhetorical flair he asks:

What could an information-processing device really tell anything (including another component in a sub-personal . . . informational system)? It is essential to realize that the answer to this question can be—in fact is—“Nothing,” without the slightest threat being posed to the utility, or even the theoretical indispensability, of cognitive science. (350)

In the surrounding passages McDowell makes it utterly clear that even though the so-called information processing systems are only syntactic and not semantic engines, talking ‘as if’ such systems are semantic engines answers an important explanatory need. This is why, for him, admission of the literal nonexistence of subpersonal contents in no way detracts from the “rich promise of cognitive science” (356). In this light, McDowell appears to be advocating a kind of fictionalism about subpersonal contents in a way that might be used to support FaF in this domain.

But McDowell’s strategy prompts the question: Exactly what kind of explanatory value might these purely metaphorical stories about subpersonal goings-on have? Certainly, talk of subpersonal contents cannot *literally* explain cognitive activity. For, if so, the McDowellian line would be incompatible with the antirepresentationalist turn in cognitive science and it would suffer from the same problems that afflict the more straightforward instrumentalist line discussed above.

To understand McDowell it must be observed that in allowing that talk of sub-personal contents has ‘theoretically indispensable’ explanatory power, he is not claiming that such talk has explanatory power in the scientific sense. Rather he holds it has explanatory value in a philosophical sense.³ For him, stories about the manipulation and production of subpersonal contents serve as a kind of conceptual bridge between two domains; it allows us to see an intelligible connection between subpersonal happenings that enable cognition—but which don’t literally involve dealings with contents—and personal level happenings, that do, literally, involve dealings with content. For this reason he tells us:

nobody knows how to *make sense of* an animal's internal control mechanism, and *how to connect it conceptually* to the competence it is supposed to explain, except by describing it *as if* it were, what we know it is not really, a semantic engine . . . (351, first two emphases added)

The kind of 'as if' talk of which McDowell speaks allows us to see an intelligible conceptual connection between domains, but in fact no real connection of an intelligible sort exists between them. Again, McDowell does not hold that talk of contentful representations figures in serious scientific explanations, only philosophical ones. But once this is revealed, it is hard to see what could justify cognitive scientists believing in such contents. On the assumption that cognitive science is only driven by scientific ambitions, as soon as it is admitted that talk of subpersonal contents can in no way figure in *serious* enabling explanations—neither realist nor instrumentalist—it seems that the only honest conclusion is that telling such stories only has the explanatory force of telling modern-day myths.⁴ But myths are surely not theoretically indispensable.

2. Fictionalism about Folk Psychology: Take Two

Some philosophers deny that folk psychology needs any vindication from cognitive science. For those who do it is a mistake to assume that the outcome of debates in cognitive science will determine the fate of folk psychology. By their lights the classical cognitivists and eliminativists described in the previous section are really only placing bets on the likelihood that a certain *picture* of the subpersonal mechanics of the mind derived from folk psychology will find a place in mature sciences of the mind (Hutto, in press).

Philosophers of this stripe think it is misguided and presumptuous to assume that in making sense of actions in terms of reasons the folk are thereby committed to the existence of subpersonal mental representations of the sort that lie behind and cause actions by mechanically generating them (see Hutto 2011).⁵ Despite its seductive attractions the idea that the folk have such commitments about the nature of minds is not supported by careful attention to our everyday practice of making sense of actions in terms of reasons, a major part of which involves the attribution of mental attitudes.

This becomes evident if folk psychology is understood not as an implicit theory but instead as a kind of shared practice.⁶ This is at the heart of approaches to understanding what the game of giving and asking for

reasons involves as advocated by Bruner (1990) and Brandon (1994). Inspired by such views I have argued in other publications that (i) our capacity for understanding actions in terms of reasons is best characterized as a kind of narrative practice and that (ii) acquiring the capacity for supplying and digesting reason explanations might (at least normally) depend upon having a special training with narratives (Hutto 2008a, 2009).

Andrews has argued that this characterization of folk psychology leads naturally to or otherwise encourages FaFish anti-realism. She writes:

Hutto's view . . . suggests antirealism about the attitudes and the view that reasons are causes of action. As *nothing but a cultural construct*, folk psychology may be a pleasant lie that has some evolutionary advantages—a shared narrative about humans as believers and desirers who act for reasons that helps to promote social cohesion by reinforcing an in-group identification . . . folk psychology [may be] an inaccurate though useful strategy for keeping individuals safe in larger groups. (2009, 233, emphasis added)

Certainly, one way of developing the idea that folk psychology understood as a narrative practice is 'nothing but a cultural construct' or a 'pleasant lie' is to suppose that when we engage in folk psychology we are only making-believe that there are beliefs and desires. While doing so may be useful—while such a game may do important work for us—this is consistent with not taking attitude ascriptions and reason explanations too literally or seriously. The fact that we are prepared to admit, under pressure, that we might only be engaged in a fictionalist pretence when making such ascriptions is consistent with holding that the game of giving and asking for reasons plays a very important role in our life.

Surely, FaF is one possible way to go if one is inclined to view folk psychology as a kind of narrative practice, but it is neither necessary nor the best option. Before signing up to it we may want to look more closely at what motivates it. Andrews is impressed by the social psychological literature. In making her claim about the link between viewing folk psychology as a narrative practice and FaF, she cites the seminal work of Nisbett and Wilson (1977)—authors who reveal that we have a penchant for *post hoc* confabulation. Although, it remains an open and much-contested empirical question to what extent we are in fact prone to offer mistaken reason explanations of our actions, there can be no doubt that we do so.

Why did I drive to Maida Vale this past Saturday? In the abstract, there are many possible reasons that could potentially explain my action.

I may have driven to London because of an appointment; due to coercion; to honour a promise to a friend; or to avoid doing something else; and so on. Perhaps I am doing it for all these reasons at once. Facts about my personal history will delimit the set of possible explanations and rule some of them out straightaway. For example, as it happens, I was not coerced into taking the trip to London nor did I make any promises to do so to a friend.

But couldn't it also turn out that I am just wrong about why I went? Mightn't I have taken the trip for no reason at all? Perhaps I drove to London out of sheer force of habit and not for any of the reasons just cited. I might favour one or several reason explanations for my action and it might be that I am, in fact, mistaken or worse self-deceived about why I acted, or even that I acted for a reason at all. We are convinced that this is so when faced with an account of someone's reason for action (or the cause of their behaviour) that is (i) at once superior to and incompatible with the person's preferred explanation and (ii) fits with the relevant facts about their personal history.

For example, imagine that a friend, F, avows that her reason for going to a particular cinema is that it has started showing art-house films. But suppose you have ample prior evidence that on the whole F despises such films and that she would normally avoid that particular cinema because it is in a bad part of town. Of course, one has to make allowances for changes of mind. But suppose you also discover there is good evidence that F has developed a special but unacknowledged attraction for one of the ticket sellers at the cinema. Moreover, suppose F continues to visit the cinema in question even after it ceases to show art-house films. In such a case, there are strong grounds to doubt F's preferred explanation of her actions. It may be that F was knowingly dishonest about her reasons, or perhaps she was self-deceived. What are the options for making sense of F's behaviour? Andrews is correct to note that on my account it might be that F acted out of a quite different set of propositional attitudes (the explicit desire to see the ticket seller and the belief that he would be at the cinema at the relevant times). Or it might be that, on my account, F did what she did on the basis of intentional attitudes directed at that situation but which lacked content. In either case, F's official reason explanation of her action would be incorrect. Admittedly, there may be plenty of cases in which a person's preferred account of their reasons fits this sort of pattern.

Yet even assuming that we, perhaps quite often, confabulate in ascribing attitudes and reasons on particular occasions—i.e., allowing for the sake of argument that we are very heavily prone to think we are acting for a reason when in fact we are not—does not establish what Andrews requires to establish: a strong link between my view that folk psychology is a narrative practice and antirealistic FaF. To show that she would need to show that there is no question of being right or wrong when giving reason explanations. However, the mere fact that we can be wrong in giving reason explanations points in the opposite direction; it implies the possibility of getting things right.

What this highlights is the quite general truth that not all narratives are fictions. Indeed, “the possibility of truth in factual narratives, as contrasted with fictional narratives, [is] relatively straightforward . . . Roughly, a proposition in a narrative will be true if it corresponds to the facts. Thus, if I say that I didn’t steal the sweets, what I say will be true, just if things were, in fact, as I say they were” (Goldie 2004, 120–22). Exactly the same holds for ascriptions of mental attitudes offered in folk-psychological narratives. So, *pace* Andrews, there is not a natural or obvious link between thinking of folk psychology as a narrative practice and FaF.

3. *Dennettian Motivations*

We are not yet out of the woods. A seemingly stronger way to motivate a FaFish line is to raise familiar concerns about the indeterminacy of folk-psychological ascriptions. This is something that anyone attracted to the view that folk psychology is some kind of narrative practice ought to address.

Dennett’s writings (1985; 1987; 1991a–c) contain arguments that are meant to encourage FaF. Famously, he introduces a distinction between various types of predictive and retrodictive stances that we can adopt towards phenomena. He distinguishes the physical, the design and the intentional stances.

Adopting the physical stance, according to Dennett, allows for perfect results—in principle.⁷ To take up this stance in its ideal form would be to make use of a completed physics that trades in nonprobabilistic laws. That would, in principle, allow for the prediction of future events or the retrodiction of prior events with unerring certainty, assuming that one has relevant knowledge of the current state of the universe. Only from the per-

spective of the physical stance so conceived would it be possible to adjudicate decisively between rival proposals.

Even in the limit the same is not true, according to Dennett, for folk psychology. By his lights, folk psychology is the adoption of a fundamentally different type of stance, the intentional stance. Taking up the intentional stance enables pragmatically valuable predictions and explanations of a kind that would be unavailable from the physical or any other stance. But, by direct comparison with what is gained by adopting the physical stance, Dennett holds that it would be a mistake to assume that (i) there are deep matters of fact underpinning the truth of folk-psychological attributions and that (ii) folk-psychological predicates refer to entities that are as real as those described by physics.

Folk psychology is, for Dennett, a type of practical calculus or heuristic for predicting behaviour—one that can be used to predict the behaviour of many types of system (Dennett 1987, 206). From the folk-psychological perspective almost anything can be treated as an intentional system—as having beliefs and desires. This is true for thermostats, rivers, lightning bolts, and people. Still, there are limits. Dennett draws the line at using the intentional stance to predict and explain the behaviour of lecterns and other inanimate things. For in order to treat something as dull and unadventurous as a lectern as an intentional system we would need to tailor its “beliefs and desires in a quite unprincipled way” (Dennett 1987, 23).

The reason that so many and various systems can be regarded as having beliefs and desires is because, for Dennett, all there is to having such attitudes is that an intentional stance description be successfully predictive of you. As he says:

all there is to being a true believer is being a system whose behaviour is reliably predictable via the intentional strategy, and hence all there is to really believing that *p* (for any proposition *p*) is being an intentional system for which *p* occurs as a belief in the best (most predictive) interpretation. (Dennett 1987, 29)

The trouble is that, if Dennett is right, when it comes to folk psychology “there will never be a ‘best’ interpretation” (McCulloch 1990, 3). This is the crux of the matter. For although Dennett allows that folk-psychological attributions are objective or real patterns that we can recognize, he adds that “these objective patterns fall short of perfection, as they always must, there will be uninterpretable gaps; it is always possible in

principle for rival intentional-stance interpretations of those patterns to tie for the first place, so that *no further fact could settle* what the intentional system in question really believed" (Dennett 1987, 40, emphasis added).⁸ Crucially, Dennett is "denying that there are *deep facts* concerning what people really believe or desire" (McCulloch 1990, 3).

These observations inspire Dennett, at least in some moods, to suppose that the predicates of our folk-psychological calculus should be treated as no more, or less, real than the numbers we use in ordinary calculus (Dennett 1987, 72).⁹ Our folk-psychological calculus provides us with a means to make rough and ready predictions about the behaviour of systems—however, if we want to know what kind of things there 'really are' we must turn to the underlying physical descriptions, on the assumption that only the kinds described by physics are real kinds.

Although Dennett bills himself as a realist, his views about how exactly we should understand the ontology of folk psychology are notoriously hard to pin down. His caginess about the ontological status of the mental is made clear in the following quotation: "the definition of intentional systems I have given does not say that intentional systems *really* have beliefs and desires, but that one can explain and predict their behaviour by ascribing beliefs and desires to them" (Dennett 1985, 7, emphasis added).

In promoting this line about the reality of folk-psychological phenomena Dennett is often regarded as advocating a brand of instrumentalism, though he prefers the label mild realist (Dennett 1991a). Thus, when rhetorically musing 'Are pains real?' he answers: "They are as real as haircuts and dollars and opportunities and persons, and centers of gravity, but how real is that?" (Dennett 1991c, 460). Presumably his answer is they are a bit real, semi-real, or quasi-real.

Is Dennett a FaFer? His fictionalist tendencies are clear and strong in his treatment of phenomenal consciousness. For him, taking talk of phenomenal consciousness as seriously as it can be taken requires adopting the intentional stance towards what others say about their phenomenally conscious experiences. The basic data for investigating consciousness is what others say—the reports they give. These reflect the judgments they make and beliefs they hold about how things *seem* to them. But it is possible to take very seriously what is said in such cases without having to assume that the person's reports about how things seem to them are based on acquaintance with real inner mental items such as qualia. This is

because—as is the case with taking up the intentional stance quite generally—we need not assume that in providing coherent chat about some topic the folk are in a position to know what really exists.

We can allow subjects to verbally describe their phenomenal states of mind—and take those reports seriously—while remaining steadfastly neutral about the ontological status of what is described. At this stage of the analysis, accordingly:

The subject's heterophenomenological world will be a stable intersubjectively confirmable theoretical posit, having the same metaphysical status as, say, Sherlock Holmes' London or the world according to Garp. (Dennett 1991c, 81)

This is Dennett's way of treating first-person deliverances about phenomenal experience by making use of the intentional stance. But what if we turned the intentional stance on itself? Would doing so result in FaF? Mightn't we take talk of folk-psychological phenomena seriously as a region of discourse without assuming that in so doing we are committed to the existence of intentional stance entities—e.g. beliefs and desires—as having any more reality than the populace of Holmes' London?

Dennett denies that he is a fictionalist about folk psychology because he takes fictionalism to be the view that “certain theoretical statements are *useful falsehoods*” (1987, 72). In rejecting this and the idea that folk-psychological statements are neither true nor false, Dennett sees himself as rejecting FaF. Instead, he admits that psychological ascriptions can be true but he immediately qualifies this by telling us that, in such cases “truth . . . must be understood with a grain of salt” (72–73). Thus if we operate with the broader understanding of fictionalism that Sainsbury provides—as discussed at the opening of this essay—Dennett is a FaFer after all: in talking the talk of mild realism he wants to believe in beliefs without committing to their full metaphysical reality.

4. Questioning Dennettian Motivations

First things first. There are reasons to doubt that the contrast between what is mildly real and what is really real holds water. Perhaps there is no coherent contrast to be intelligibly drawn between that which is fully real and that which is somehow less than fully real.

Here it helps to be reminded about how understanding actual fictions presents us with puzzles about how to make sense of facts and truths that are restricted to special contexts.

There are *real facts* about what is said to happen in a story, even if the story is just a story and recounts nothing real . . . According to the Sherlock Holmes stories, Holmes played the violin. That is true, but it is not true that Holmes played the violin. (Sainsbury 2010, 2, emphasis added)

One way to make sense of truths in fictions is to introduce a dualism of types of truths. This would provide a way of respecting the fact that “people don’t think that ‘Holmes lived on Baker Street’ is *really* true” (Sainsbury 2010, 26, emphasis original). To accommodate truths in or about fictions it might seem that we are justified in distinguishing between genuine truths and qualified truths—truths of a special sort that are only true relative to a context. Thus one way of dealing with such cases, as Sainsbury (2010) describes, would be “to recognize, in addition to a conception of absolute truth, a conception of truth relative to a presupposition or a premise” (27). However, insofar as talk of absolute versus relative truths is anything other than metaphysically innocent shorthand for the fact that we implicitly operate with a fiction operator when dealing with fictions as opposed to making default and unspoken assumptions about the claims that are made in nonfictional contexts, we should resist this temptation.

Surely there are genuine truths and facts about fictions, *once* the relevant claims are appropriately contextualised.

1. Did Sherlock Holmes live at 221b Baker Street in London?
2. Did Sherlock Holmes live at 221b Baker Street in a counterpart of Victorian London?
3. Did Sherlock Holmes ever discuss logic with a student of Frege?
4. Did Sherlock Holmes have Asperger Syndrome?
5. Is Sherlock Holmes a popular fictional character?

As they stand, questions 1 and 2 are ambiguous. Not only do we need to isolate the question to a fictional domain, we need to know which

fiction or fictions—i.e., which stories—we are asking about. In some stories Holmes, for example, lives in a counterpart of contemporary London. Nowadays he even lives in a counterpart of contemporary New York. In one story, at least, he did discuss logic with a student of Frege's, so if we restrict the question to that story then the answer to 3 is true. Stepping outside of the fictional context, question 5 is easily answered, but we may have qualms about what to make of question 4. But if we have worries about whether to take it seriously this will be because we have no readily available procedure, as in the case of question 5, for answering it.

There is nothing special here. As in all other cases, the truth-value of sentences depends on appropriately prefixing and contextualizing their content. We need to know which language is being spoken, what the scope of the claim is, and so on. After such details are ironed out, 'P' will be true if and only if things are as 'P' says they are. This is true right across the board—both in fictional domains and in all other domains, including folk psychology and physics.

Of course, in every case we can ask further questions about what makes it the case that P obtains. And here different sorts of facts are appealed to in line with our procedures for answering such questions. Thus the fact that Holmes lives at 221b Baker Street in the Conan Doyle stories depends on its being the case that Conan Doyle stipulated that it was so. That Holmes is a popular fictional character depends on facts about the fiction-consuming public's reaction to him. That I drove to London last Saturday because I believed I needed to update my passport with a visa is made true by facts about my personal history and the attitudes I held at that time.

What follows? Well, if there can be determinate truths and facts even in and about fictions this should raise suspicions about drawing a distinction between what is really real and what is only mildly real in the folk-psychological (or any other) domain. Quite generally, there are matters of fact but there are no 'deeper' matters of facts, no 'truer' truths, no 'really' reals. To accept this undermines any FaFish attempt to accommodate Dennettian worries that folk-psychological phenomena might be less than fully real. The crux is: If we cannot draw a distinction between the really real and the mildly real even in the domain of fiction then we might doubt the sense of doing so in the domain of folk psychology. Put pithily, we must "drop this distinction between first- and second-class truth between sentences which express 'matters of fact' and those which do not" (Rorty

1987, 116). We are entitled to do so if the very idea of a contrast between what is fully and only mildly real is inherently confused.

To be real to any degree or extent is to be real *simpliciter*. That, of course, leaves open lots of further questions about the way in which something in any given domain is real—i.e., which sorts of properties it must, or can, have or lack. But that is coherent with a rejection of mild realism as unstable and incoherent. It looks then like the only option for any given domain boils down to full-blooded realism or antirealism.

Assuming this, it might seem in the light of Dennett's worries that perhaps the smart money is for those attracted to FaF to slide towards eliminativism and "replace talk of fiction by talk of myth" (Sainsbury 2010, 206). Perhaps the idea that there are any folk-psychological phenomena—even those mentioned in explanations of what people do—is just a myth.¹⁰

Thoroughgoing eliminativism about folk psychology is not a live option for anyone who, like Dennett, accepts the reality of contentful stances. Having removed the possibility that stances might be mildly real, we must ask—more simply—are they real stances or not? The question for folk psychology is the traditional Shakespearean one: to be or not to be?

If these are the only options, followers of Dennett must opt for the first. Indeed, Haugeland (1993) takes Dennett to harbour uncompromisingly strong realist commitments about the status of stances. He writes:

A *stance*, on the face of it, is a kind of posture or attitude that somebody can take toward something, a specific way of regarding and dealing with it. That intentional systems should be defined in terms of a possible attitude towards them has misled many into thinking the point is to downgrade their status, to imply that the intentional is somehow secondary, observer relative, or artificial—in contrast, say, to the physical. But, in this respect, Dennett puts the intentional and the physical exactly on a par: each is understood in terms of a possible stance.¹¹

Of course, it is possible to draw a distinction between stances and the subject matter of stances—i.e., what they are stances towards. And as long as these can be prised apart it is possible to maintain a differential attitude toward the ontology of the mental and the physical; for these can be understood as distinct domains or subject matters. Yet it is not open for those who accept Dennett's system to doubt the reality of stances. And if stances just are contentful attitudes of some folk-psychological sort—and assuming they are irreducible to purely physical phenomena—then it looks as if the reality

of folk-psychological phenomena is secure. In light of the above considerations they will require more than mild realist or fictionalist treatment.

And there is something else worth mentioning at this juncture. Picking up on a related issue, Sainsbury questions the general logic of the fictionalist strategy—that of trying to make metaphysical savings by assuming that the ontology of some region of discourse is best understood by the fact that we tell fictional stories about it. In a nutshell Sainsbury's worry is this:

No progress towards nominalism has been made . . . The adequacy of irrealism may be necessary but it is not sufficient to satisfy full-blown nominalist aspirations. Mathematics is a fiction, a story. But what is a story? The most natural answer is that it is something abstract: something which can be told on different occasions, or written down in different copies and different languages. Nominalists have to account for fictions in terms of concrete tokens. I don't envy them. (Sainsbury 2010, 2)

Fictionalists push their ontological debts from one internal budget to another. Yet, given that such debts must always be paid in the end, this looks like a short-term tactic rather than a viable long-term strategy.

This is all very well. But what then should we say about Dennett's worries about the indeterminacy of folk-psychological attributions? He tells us that:

The bogey of radically different interpretations with equal warrant from the intentional strategy is theoretically important—one might better say *metaphysically important*—but practically negligible once one restricts one's attention to the largest and most complex intentional systems we know: human beings. (Dennett 1987, 29, emphasis added)

It is certainly true that folk-psychological attributions are indeterminate—in that they always admit of equally good rival interpretations—*when* we engage in certain kinds of speculations about other minds for the purpose of third-personal prediction and explanation. This is a fact about one way of using folk psychology. But this fact does not have the metaphysical implications that Dennett claims it has.

This would only follow if, *in essence*, folk psychology were in the same basic business as physics, that of making third-personal predictions and explanations. Many take this for granted—whether explicitly or implicitly. For example, on the basis that folk psychology does not trade in strict, exceptionless laws Davidson infers that “reason-explanations . . .

are in some sense low-grade; they explain less than the best explanations in the hard sciences" (Davidson 1987, 42; see also Davidson 1990, 7). Of course, this comparison is confused if Davidson is right that folk psychology is irreducibly normative. To assume the latter is to assume that folk psychology is fundamentally unlike physics; that is, it has different operating principles and does different kinds of work. If so, comparing the two kinds of explanations in the way described makes as much sense as comparing chalk and cheese.

Why assume that folk psychology is, *essentially*, in the business of providing third-personal predictions and explanations? Focusing on the regulative aspects of folk psychology, McGeer offers an important corrective to the reigning view about the primary function of folk psychology—i.e., the idea that it functions like a proto-scientific theory. She observes that if we are in the thrall of this assumption:

we overlook the way folk psychology operates as a regulative practice, moulding the way individuals act, think and operate so that they become well-behaved folk-psychological agents: agents that can be well predicted and explained using both the concepts and the rationalising narrative structures of folk psychology. (McGeer 2007, 139)

Building on this, she concludes that:

Our capacity for 'explaining and predicting' others' behaviour can then be understood more fruitfully in light of this account—viz., as a capacity that is exercised in more limited ways than envisioned by the standard approach, and as a capacity that has its ups and downs but *which cannot be assessed, in any case, according to the standards of explanatory/predictive practices in the sciences*. (McGeer 2007, 139, emphasis added)

These observations dovetail with a quite different story about the basic function of folk psychology—one that takes it that reason explanations function, *primarily*, as normalizing explanations. When I am interested in the reason why my wife was wandering in a graveyard at 3 A.M. in the morning, I am not concerned with explaining her behaviour in the sense of being able to retrodict it. What I want is an account—a true account—that makes sense of her actions. Crucially, folk-psychological explanations in such cases are meant to be like historical explanations. They are not third-personal speculations that can only be justified by their predictions or retrodictive successes. Rather such explanations take the form of narratives that are concerned with idiosyncratic details of a

person's history; details that are personal and particular. Importantly, folk-psychological narratives that actually explain—those that are true—are precisely unlike fictions (see Sainsbury 2010, 4); they seek to get at facts that are a matter of historical record and, if successful, they get a handle on a person's actual attitudes. I leave aside the question of which methods are most reliable for getting at such facts; it suffices to stress here that this is the primary business of folk psychology.

Dennett holds that “the pattern of belief is discernable in an agent's (observable) behaviour when we subject it to ‘radical interpretation’” (Dennett 1991a, 30). But in going wholly third-personal in this way, and assuming this is the essence of folk psychology (as opposed to just one possible way of using it), Dennett adopts—without warrant—a limited criterion for assessing the determinacy of folk-psychological attributions. This is why, in turn, he thereby underappreciates the ways in which folk-psychological patterns are unstintingly objective. The mistake here is to assume that folk psychology is fundamentally in the business of providing third-personal predictions and explanations. This is to misunderstand what lies at the roots of the practice and the role that folk psychology plays in our lives.

In conclusion, when it is important to choose between rival folk-psychological attributions we try to get at facts about the detailed personal history of individuals in a principled and nonarbitrary manner. Our methods—especially when applied in quick and dirty ways—fall short of perfection, mistakes are always possible. But Dennett's observations provide no reason for thinking that, in principle, if we had all of the relevant facts before us, we would be always unable to decide between certain rival mentalistic ascriptions.

The ultimate source of trouble—that which drives Dennett into the arms of FaF—is his tactical choice of starting point. For him, like Quine before him, it is “the objective, materialist, third-person world of the physical sciences” (Dennett 1987, 5). This leads Dennett to adopt an untenable double standard about the reality of folk psychology. For Dennett feels the force of the Quinean injunction that:

If we are limning the true and ultimate structure of reality, the canonical scheme for us is the austere scheme that knows no quotation but direct quotation and no propositional attitudes but only the physical constitution and behaviour of organisms. (Quine 1960, 221)

But we need not start here. We might instead join with Wittgenstein in thinking that what is given is forms of life (1953, 226e). We might thereby get beyond physicalism, accepting that physics is not metaphysically privileged and thus remove any warrant for trying to downgrade the reality of folk-psychological phenomena. If we did, it would be safe to conclude that folk psychology is a narrative practice—one that surely includes the telling of occasional fictions and occasionally just getting things wrong. But, taken as a whole, there is no good reason or basis for understanding folk-psychological discourse and what it ascribes in fictionalist terms.¹²

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NOTES

1. Eliminativism in this domain must be motivated by nonrepresentational developments in cognitive science. It is a mistake to conclude that folk psychology could be falsified because “neuroscience is unlikely to find ‘sentences in the head’ (Churchland 1991, 65). What is at stake between eliminativists and their opponents cannot be decided by neuroscientific findings. For it would always be open for friends of folk psychology to argue that the vehicular processes at the neural level that make cognition possible are entirely nonclassical—viz. from the neuroscientific point of view they look nothing like the crunching of contentful symbols—even though they are in fact a form of symbol processing. Thus neuroscientific considerations for rejecting folk psychology, such as Churchland’s, simply miss their mark; they target the wrong level of explanation. However, it is a very different story if it turns out that there are no subpersonal contents. Then the game is really up. For to abandon the idea that subpersonal cognition is fundamentally content involving threatens, for obvious reasons, the vehicle/content distinction itself: no content, no vehicles of content.

2. Churchland does not mince words: “should we ever succeed in making the shift away from folk psychology, we shall be properly at home in the physical universe for the very first time” (1979, 35).

3. It is useful to compare what McDowell has to say on this score with what Dennett says when he makes a case for thinking that some version of representationalism is true while recognizing that this is—at best—a contingent and scientifically discoverable matter of fact (see Dennett 1987, 34–35).

4. After all, as Sainsbury reminds us:

Myths are typically not propounded as myths, but start life propounded and accepted as truths. Should we regard them as fiction at that early stage? Presumably not. They are taken too seriously. Recountings of them are intended to reinforce belief not make-belief. They start to be counted as myths when they cease to be believed . . . [when they are] transformed from misguided factual narrative to resonant fiction. (Sainsbury 2010, 23)

5. Reason explanations are nothing like the causal hypotheses given by scientists who advance engineering hypotheses about the workings of the brain. This is not to deny that in citing a reason we pick out something that carries causal information in some weaker sense, e.g., in the sense of tracking something that makes a difference to our actions.

6. This goes significantly further than Dennett's observation that "a good reason for not calling [folk psychology] a theory, [is that] it does not consist of any explicit theorems or laws" (Dennett 1991b, 134).

7. He says, "when we adopt the physical stance toward a configuration . . . our powers of prediction are perfect" (Dennett 1991a, 38). And elsewhere, "The strategy is not always practically available, but that it will always work in principle is a dogma of the physical sciences (I ignore the minor complications raised by subatomic indeterminacies of quantum physics)" (Dennett 1987, 16).

8. Or again:

I see that there could be two different systems of belief attribution to an individual which differed substantially in what they attributed—even in yielding substantially different predictions of the individual's future behaviour—and yet where *no deeper matter of fact* could establish that one was a description of the individual's real beliefs and the other not. (Dennett 1991a, 49, emphasis added)

It is an exactly similar story with respect to the design stance. The design stance is a heuristic that enables us to predict and explain the behaviour of a system based on assumptions about its proper functions. But here too our powers of prediction and explanation fall short of perfection. Design stance attributions are also indeterminate. Although we can make predictions and explanations about a system's likely behaviour based on assumptions about its function or design, these will be necessarily less powerful than those made from the physical stance. This is because the design stance assumes that systems will operate as they are meant to (i.e., in accordance with their design). For this reason malfunctions and breakdowns are invisible from its perspective. So, here again, Dennett concludes there are no deep matters of fact about which design stance ascriptions are correct. This is quite unlike the situation that would be afforded to us if we adopted the physical stance.

9. In other moods, he goes so far as to say that we may discover that some version of the language of thought hypothesis is true. This will be so if it turns out—as a matter of fact—to provide the best explanation of why intentional stance predictions work. But in pressing this line, Dennett insists:

It is not that we attribute (or should attribute) beliefs and desires only to things in which we find internal representations, but rather that when we discover some object for which the intentional strategy works, we endeavor to interpret some of its internal states or processes as internal representations. What makes some internal feature of a thing a representation could only be its role in regulating the behavior of an intentional system. (1987, 32)

10. Ratcliffe (2007; 2008; 2009) has argued strenuously that folk psychology is best understood as a philosopher's myth and hence should be eliminated. He claims that: "people do not employ a belief-desire psychology at all" (Ratcliffe 2009, 386); that "FP is not simply 'false' but . . . it is so abstract and uninformative that it warrants rejection rather than revision" (Ratcliffe 2009, 386); and FP does not reflect "in any illuminating way, the structure of everyday interpersonal understanding" (Ratcliffe 2009, 382). I have counter-argued that once we correct for certain common misconceptions about the nature of folk

psychology there is no need to reject it in this way (Hutto 2008b). Ratcliffe acknowledges that my account of folk psychology as a narrative practice differs from the standard characterization of folk psychology and abandons much that is objectionable. Nevertheless, he maintains that my proposed adjustments do not go far enough. In particular, he holds that "Through force of philosophical habit perhaps, Hutto still sees a belief-desire garden through the weeds that he has rightly let grow over FP" (Ratcliffe 2009, 402).

11. Haugeland (1993, 65). Accordingly:

It would be a misunderstanding . . . to take the analogy between intentional states, on the one hand, and patterns and game phenomena, on the other, as a way of mitigating or watering down the ontology of the mental—as if to say they're 'no more real' than these. (64)

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